lectual faculties of the highest order, such as none of his descendants have surpassed. His speech, we may be sure, was not a mere mumble of disjointed sounds, framed of interjections and of imitations of the cries of beasts and birds. It was, like every language now spoken anywhere on earth by any tribe, however rude or savage, a full, expressive, well-organized speech, complete in all its parts. The first men spoke, because they possessed, along with the vocal organs, the cerebral faculty of speech. As Professor Max Müller has well said, "that faculty was an instinct of the mind, as irresistible as any other instinct." It was as impossible for the first child endowed with this faculty not to speak, in the presence of a companion similarly endowed, as it would be for a nightingale or a thrush not to carol to its mate. The same faculty creates the same necessity in our days; and its exercise by young children, when accidentally isolated from the teachings and influence of grown companions, will readily account for the existence of all the diversities of speech on our globe.

WHAT IS NERVE-FORCE?

A DISTINGUISHED biologist has remarked, with great truth, that the study of the nervous system is the true field of battle for physiologists, all other investigations, however interesting and important, being of the nature of skirmishes, preparatory for and surely leading up to the final conflict, in which we must engage before we can hope to gain a position from which nature's most mysterious processes are laid bare to our view. Of all the functions of the nervous system, the one which, at first sight, would seem most accessible to investigation, is that of the nerve-fibre itself. What conception can we form of the physical or chemical changes which take place in those white glistening bands which are for us the only channels through which knowledge of the physical universe can be obtained, and which also enable us to impress upon the world around us the evidence of our conscious personality?

With the discoveries of Du Bois Reymond, the hope arose that nerve-activity might be explained as an electrical phenomenon, and the attempts made to build up a satisfactory electrical theory of nervous action have been numerous and ingenious. The important facts which forbid the identification of nerve-force with electricity are: the absence of an insulating sheath on the nerve-fibre, the slow rate at which the nerve-force is transmitted, and the effect of a ligature on a nerve in preventing the passage of nerve-force, while not interfering with that of electricity. The electrical phenomena connected with the functional activity of nerves (action-current, electrotonus) appear, therefore, to be secondary in their character, and not to constitute the essential process in nerve action. In this connection should be noted an experiment of d'Arsonval,[1] which shows how the electrical phenomena associated with the activity of nerves may be imitated by purely physical means. This observer filled a glass tube, of one or two millimetres interior diameter, with drops of mercury alternating with drops of acidulated water, thus forming a series of capillary electrometers. The tube was closed at its two ends with rubber membranes, and was provided with lateral openings by which its interior could be connected with electrical conductors. A blow upon one of the membranes caused an undulation of the liquid column, which was propagated from one end to the other of the tube, and was accompanied by a wave of electrical oscillation, which was propagated at the same rate. The phenomenon is, according to d'Arsonval, to be explained as follows: The blow upon the membrane changes the form of the surface of contact between the first two cylinders of mercury and acidulated water. This change of form is transmitted to the following cylinders with a rapidity dependent upon the nature of the fluid. But each of these changes of shape is accompanied by the production of an electric current (Lippmann's phenomenon, due to variation of superficial tension), and the tube is therefore traversed by an electric wave, which necessarily has the same rate as the undulation of the liquid column. The analogy between this phenomenon and the wave-like propagation of the action-current in nerves is sufficiently obvious.

In studying the nature of nerve-force, two alternatives present themselves. We may conceive the impulse to be conducted through the nerve-fibre by a series of retrograde chemical changes in the successive molecules of the nerve-substance, the change occurring in one portion of the fibre acting to produce a similar change in the neighboring portion. As this process is associated with the using up of organic material, and the consequent discharge of potential energy in the successive portions of the nerve, the theory may be called 'the discharging hypothesis.' The burning of a line of gunpowder may be taken as an example of this sort of action. On the other hand, we may conceive that the nerve-force is transmitted from molecule to molecule by some

Abstract of an address before the section of biology of the American association for the advancement of science at Buffalo, Aug. 19, 1886, by Dr. H. P. Bowditch, of Boston, Mass., vice-president of the section.

[1] *Comptes rendus soc. biol.*, April 3, 1886.

sort of vibratory action, as sound is transmitted through a stretched wire. As this theory does not involve the using up of any material, but simply the transferring of motion, it may be called 'the kinetic hypothesis.'

Inasmuch as the discharging hypothesis involves the destruction of organic material, we may, if this theory be correct, reasonably expect to find in the active nerve-fibre evidences of chemical decomposition and of heat production. Moreover, if the organic substances are used faster than they are replaced, or their products of decomposition removed, as they would naturally be under constant stimulation, we may expect to observe a diminution of nerve-action during the continuance of the stimulation; in other words, we shall have the phenomena of fatigue. On the kinetic hypothesis, on the other hand, we may expect to find an entire absence of chemical decomposition and fatigue, and, if the moving particles are endowed with perfect elasticity, an absence also of heat production.

The only functional chemical change of nerves for the existence of which an experimental proof has been offered, is the change in the reaction with test-paper. Just as the normally alkaline tissue of muscles becomes neutral or acid in activity, so, according to Funke[1] and Ranke,[2] do nerve-fibres and the white substance of the spinal cord change in activity from an alkaline to an acid reaction. Liebreich[3] and Heidenhain,[4] on the other hand, experimenting with a slightly different method, failed to get any evidence of the acidification of nerves in connection with functional activity. The phenomenon must indeed be a delicate one, since Ranke himself urges that the question should be decided by experiments on the spinal cord, and should not depend upon the 'doubtful results of tests applied to the nerve-trunks.' Now, since the cord contains gray as well as white substance, and as the gray substance, according to Ranke himself, becomes more acid than the white in functional activity, it is clear that an acid reaction of the white substance of the spinal cord may depend upon an acid formed in the gray and passing by diffusion into the white substance. This possibility, which is indeed admitted by Ranke, seems to deprive the experiments on the spinal cord of what little value they possessed as evidence of the production of acid in connection with the activity of nerve-fibres.

The other chemical changes which have occasionally been asserted to occur in active nerves, rest on still weaker experimental evidence, and it is therefore clear that chemical investigation gives us but little reason for maintaining a discharging, in opposition to a kinetic, theory of nerve action.

The first experiments to test the heat-production of active nerves were those of Helmholtz,[1] who, after studying the analogous phenomenon in muscles, extended his investigations to nerve-fibres. He failed, however, when all sources of error were carfully avoided, to obtain any evidence of heat-production in connection with nervous activity, though his apparatus was capable of registering a change of temperature of 0.002° C. Similar negative results were obtained by Heidenhain.[2] On the other hand, Valentine,[3] Oehl,[4] and Schiff[5] maintained that nerve-fibres really are warmed by the passage of the nerve impulse. It seems, then, that the results of thermometric investigations speak no more positively than those of chemical research in favor of a discharging rather than a kinetic theory of nerve action.

The evidence of the activity of a nerve may be either direct or indirect. The direct evidence consists in the occurrence of that change of the electrical condition known as the 'negative variation,' of Du Bois Reymond, or the 'action-current,' of Hermann. The latter writer quotes the former as authority for the statement that this phenomenon becomes less intense in successive repetitions of the experiment, and regards this as evidence of the exhaustion of the nerve-fibre. Unfortunately, Hermann does not refer to the exact passage which contains this statement, and an examination of the chapter on the negative variation of nerves, in Du Bois Reymond's 'Untersuchungen,' fails to show any systematic study of the effects of fatigue on this phenomenon.

The indirect evidence of the activity of a nerve consists in the effect which it produces upon the central and peripheral organs with which it is connected. Of these effects, the contraction of a muscle is the one which is most conveniently observed, but the fact that a muscle is more readily exhausted than a nerve, renders it impossible to study the fatigue of nerves in this way without some special modification of the experiment.

Bernstein[6] was the first to employ the muscular contraction in experiments on the exhaustion of nerves. This observer finally reached the conclusion that a nerve may be exhausted by $5'-15'$ tetanic stimulation. The experiments of Bern-

1 *Arch. anat. und phys.*, 1859, 835.

2 *Centralbl. med. wiss.*, 1868, 769; 1869, 97.

3 *Tagebl. naturf. vers. Frankfurt*, 1867, 73.

4 *Studien*, iv. 248; *Centralbl. med. wiss.*, 1868, 833.

1 *Archiv. anat. und phys.*, 1848, 158.

2 *Studien*, iv. 250. 3 Moleschott 'Undersuch.' ix. 225.

4 *Gaz. med. Paris*, 1886, 225. 5 *Pflüger's archiv.*, iv. 230.

6 *Pflüger's archiv.*, xv. 298.

stein have recently been repeated by Wedenskii,[1] who was unable to find any evidence of the exhaustion of the nerve, even after the tetanic stimulation had continued six hours. A study of the subject upon warm-blooded animals seeming desirable, experiments were made upon cats, in the laboratory of the Harvard medical school.[2] It was found that stimulation of the nerve lasting from one and a half to four hours (the muscle being prevented from contracting by curare) did not exhaust the nerve, since on the elimination of the curare the muscle began to contract.

It thus appears that evidence of fatigue in nerves resulting from functional activity is as difficult to obtain as that of chemical change or of heat-production. It is conceivable that the irritability of a nerve should depend upon its possessing a certain definite chemical composition, constantly maintained by metabolic changes, and yet that the irritation of the nerve should produce no change whatever in its composition.

In support of this view, an analogy may be drawn from the physiology of the muscular system. We find here that the power of the muscles to perform their function is intimately associated with the amount of nitrogenous material undergoing decomposition in the body, but the performance of a given amount of muscular work, if within physiological limits, does not effect the amount of nitrogen excreted. In the case of muscles, to be sure, we have evidence of a considerable decomposition of non-nitrogenous material, and also of heat-production in connection with functional activity, but, if we limit our consideration to the nitrogenous element of muscular substance, the hypothesis above proposed for nerves finds its complete analogy in the muscular system.

We have thus seen that investigations into the chemical changes, the heat-production, and the fatigue of active nerves, all lead to results more favorable to a kinetic than to a discharging theory of nerve action.

We may, therefore, reasonably hope that future researches, if directed on this line, will throw further light on this most mysterious and interesting process

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IN the 'Catalogue of printed books' in the British museum, now issuing in random instalments, one heading which has just been completed — 'Academies' — is of special scientific interest. This and 'Periodical publications' (which is also nearly completed, four of the five parts being out) will indeed include reference to a large proportion of scientific literature, and it is not probable that any library in the world can at all compete with the British museum in its general completeness in these departments. The earlier publication of this list of titles of society publications would have rendered the catalogues of Scudder and Bolton more satisfactory. The volume of 'Academies' is a folio of 1018 pages. London alone occupies one part with nearly 200 pages, though Paris has less than 90. The publications are arranged under the name of the issuing body, and these alphabetically under the town where situated, the towns having their English form and making a single alphabet. Thus Compiegne, Concord, Constantina, Constantinople, and Copenhagen follow in that order. A few countries are introduced into the alphabet for some general societies, though other societies with equal right to a national name are placed under the seat of government. The United States does not appear, and it would be difficult to say where to look for our peripatetic societies. Certainly the American association publications can nowhere be found, though they are doubtless in the museum, as we note one or two other omissions known to us to be there. Only completed series are fully entered; of others, the first volume in the possession of the museum is given, with the added words, 'in progress.' There is no transliteration, but Greek, Russian, Persian, or what not, are mixed in one alphabet with the Roman. Some curious rules have been followed in the alphabetization: thus 'Société cuvierienne' precedes 'Société d'acclimatation,' because of the preposition in the latter; yet Le 'bureau des longitudes' is made to precede La 'société cuvierienne' by dropping the objectionable particles from the full names. These, of course, are minor matters, and it would take a good many such to detract in any serious measure from the value of this excellent and carefully edited work.

— Messrs. Jackman and Webster report in the *Photographic news* their results in photographing the retina of the human eye. A small camera was employed, placed behind an ophthalmoscope, and the albo-carbon gaslight was the means of illumination. In the photograph the normal cupping of the optic disk and the principal blood-vessels are readily discerned. It is evident that but a beginning has been made in this method of research; but, if continued, very valuable results may be obtained. The method of Brainerd and French in photographing the vocal cords and the interior of the larynx promises equally well, and is now employed by a number of laryngologists in making permanent records of abnormalities in these parts.

[1] *Centralbl. med. Wiss.*, 1884, 65.

[2] Bowditch, *Journal of physiology*, vi. 133.